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Interactive comment on "Seasonal dynamics of carbon and nutrients from two contrasting tropical floodplain systems in the Zambezi River Basin" *by* A. Zuijdgeest et al.

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Dear Dr. Jennerjahn,

Thank you for your efforts in reviewing our manuscript. Your detailed comments are much appreciated. Below you will find our responses to the issues you raised (paragraphs starting with »), and the way we have incorporated your suggestions into the manuscript (in parentheses). The updated manuscript (track-changes Word document) and the updated supplementary material have been combined into one file, and added to this comment as a supplement.

General comments: The authors present an interesting study on river biogeochemistry

from two Zambezi tributaries. Data and knowledge from African rivers are strongly underrepresented in the global data base and make this study a valuable addition. The study conducted in subbasins of the Zambezi River aims at assessing the effects of floodplains and dams on river biogeochemistry. Understanding of these processes in river catchments is important with regard to what ultimately reaches and affects the coastal ocean.

The paper is generally well structured and written. However, while the set of samples and analytical data appears to be robust and absolutely worthwhile publishing, the design of the study raises major concerns regarding the two major issues discussed: the effect of floodplains and dams. First, the authors collected data and samples from two stretches of the river passing through floodplains during dry and wet seasons. However, although the importance of the biogeochemical exchange between river and floodplain was stressed by the authors, samples were collected only in the middle of the river. While I agree with the authors that this exchange is important, I would also expect biogeochemical gradients from the river until the landward margin of the floodplain. The study design does not allow accounting for this. Having said this, the least the authors could do is try to find examples from the literature and discuss these.

» For both systems, we did collect samples at the edges of the channel, and as far onto the floodplain as the dense vegetation would allow us. We found there was very little variability between the middle of the channel and the edge. This data is not presented because we only have these for the wet season in the Barotse Plains. Additionally, the dense reed vegetation, and sometimes-shallow water levels did not allow movement onto the floodplain by boat. Real channel to floodplain transects were therefore not feasible. This information has now been included in the Supplementary Information, and additional floodplain transects can be found in the supplementary material of Zurbrügg et al. (2013).

Second, the authors collected data from a floodplain stretch between two dams, but have no data/samples on the respective river portions upstream of the first (ITT) and downstream of the second dam (KG) which makes it difficult to assess the dam effect, in particular with regard to quantification.

» Data upstream of ITT has been collected and discussed by Manuel Kunz, some of which has been used in this manuscript as well. However, we were more interested in the floodplain processes that might have changed due to the presence of the dam, for which we did not need data from upstream of the reservoir. Downstream of the Kafue Gorge dam we did not collect samples, because there the Kafue River flows through a steep gorge without floodplains. It was therefore not considered part of our study.

Moreover, the discussion of the important issues often remains at the surface and is full of flaws, in particular when it comes to the use of C/N ratios and stable carbon isotopes. You will find detailed comments below. While the authors have a very good set of data and the issues discussed are highly relevant, the story is simply not yet there. Therefore, this paper cannot be recommended for publication in Biogeosciences in its present form. It will require major revisions to make it a robust study and valuable addition to the literature. However, I am convinced that the authors have the means to do so and would greatly appreciate to see this study published!

» General comment regarding the reviewer's requests for more emphasis on the inferences from our study: our study is very descriptive in nature, and there is very little comparable information published for other tropical river-floodplain systems. Lacking process rates from our own work, and published context from comparable systems from the literature, we tried to focus on conclusions supported by the data.

Detail comments: Abstract: The abstract is rather long, . . .

» The abstract has been shortened from 449 to 380 words.

. . . in some places a bit confusing and ends with a little uninspired conclusions. I suggest to shorten the detail description of results and to put more effort on the inferences of the study. A final, clear take home message would be fine.

» The abstract has been more generalized, with a clear take home message at the end.

P. 10546, l. 5: Why just "hydropower" dams? There are many multipurpose dams which affect river hydrology and biogeochemistry etc.

» "Hydropower" has been removed; it can indeed be generalized here.

P. 10546, l. 11 ff.: These paragraphs are rather long and not really clear. In the first sentence you state that the "Barotse plains retain particles", but have "higher annual yields of POC and N: : :than previously reported for the Zambezi: : :". This sounds a bit strange. It must not necessarily be a contradiction, but rephrasing could help make clear what is meant.

» The abstract has been generalized, and shortened. This particular sentence has been incorporated in the new abstract in "Distinct seasonal differences have been observed in carbon and nutrient concentrations, loads, and export and retention behavior in both systems."

P. 10546, l. 23-27: There is a contradiction regarding organic matter (OM) sources. While you first stress the importance of aquatically produced OM during the wet season, you state in the following dominances of C3 vs. C4 vegetation. This does not match and raises a general issue with the use of C isotope composition. The relevance of autochthonous OM production is often neglected. For example, depending on the carbon source freshwater plankton can have the same d13Corg as C3 vegetation. Moreover, rivers do not only transport plant material, but rather larger portions of soil material. That, in turn, often has a d13Corg in between that of C3 and C4 plants.

» Rephrasing has removed the mismatch of information. The concerns regarding the use of stable carbon isotopes have been addressed with additional data and arguments in the discussion section, and the abstract has been adjusted accordingly. "Carbon to nitrogen ratios showed that soil-derived material was dominant year round in the

Barotse Plains, whereas the Kafue Flats transported aquatically produced particulate organic matter (produced in the upstream reservoir) during the wet season. Stable carbon isotopes suggested that inputs from the floodplains to the particulate organic matter pool varied throughout the year in both systems, in opposite patterns. In the Kafue Flats, encroachment of woody plants since the construction of the dams could be responsible for the altered pattern."

P. 10547, l. 7-11: So, what are the clear effects? Name them. The following two sentences are rather commonplaces. Be more specific regarding your results. It will make it more interesting for the reader.

» The commonplace sentences have been removed, and the final paragraph has been tailored to fit more to our results. "This study revealed effects of dam construction on organic matter and nutrient dynamics on the downstream floodplain that only become visible after longer periods, highlighting the need for continued monitoring after dam construction."

Introduction:

The introduction generally looks good.

P. 10548, l. 22 ff.: The first sentence is simply wrong. The type of OM never depends on discharge alone, but mainly on the sources and processes in the catchment. The following examples are quite arbitrary and not representative. Here again, we have the problem with the use of C isotopes; reducing it to C3 and C4 plants is too simple and definitely wrong!

»The first sentence has been rephrased to "… co-varies with discharge". The examples provided are of African rivers, which is the framework in which we wanted to present our results. For these examples, the use of C isotopes is cited as discussed by the respective papers. In the discussion section, additional data and arguments have been included in response to your comments.

P. 10549, l. 3-13: I suppose this paragraph should state the research problem, but remains a bit undetermined. It would benefit from clearly pointing out the gap in knowledge that this study wants to close.

» We feel confident that P10549 L6-7 states the gap: the impact of dam-induced changes in hydrographs on floodplain biogeochemistry has not been studied. We have removed P10549 L11-13, which in the previous version led to the gap becoming hidden in the middle of the paragraph.

P. 10549, l. 14-24: And following up on that, this paragraph can also be a bit shortened and focused on what was done for what purpose.

» We feel that the objective and the approach become sufficiently clear in the following sentences "Based on field campaigns from contrasting seasons, we were able to describe seasonal variability in the two systems. We further quantified the changes in the concentration, speciation, origin, and loads of carbon, nitrogen, and phosphorus along the floodplains in order to assess the implications of river damming and an altered hydrological regime on floodplain biogeochemistry."

Study sites:

P. 10550, l. 15-19: What is much more important here and later for budget calculations: was there a change in transported water volume before and after dam construction? Is the water only used for power generation or also for irrigation and/or drinking water?

» In the description of the study sites, the following information has been added after L12 (P10550): "Evaporation from the reservoir changes the water level by 780 mm year-1, according to Beilfuss (2012)." Otherwise, there is no change in the transported water volume, only a change in timing and pattern (P10550, L15-18). In the Kafue Flats there is some irrigation occurring close to the downstream end of the floodplain, in the vicinity of Kafue Town. The drinking water withdrawal is after Kafue Town, i.e. outside of our sampling area. In the Barotse Plains no organized irrigation is extracted.

Drinking water extraction from the river is minimal, as most drinking water in Zambia originates from groundwater.

Methods:

There is no information at all about data quality. Add information on accuracy/precision of analyses. This is important in order to assess the relevance of differences in numbers discussed later.

» For our laboratory analyses, the precision of the measurements was lower than the standard deviation among all the samples, as presented in Fig. 4. Moreover, we feel that adding accuracy and precision numbers for these fairly standard analyses would be too much detail and make the methods section unreadable. Precision for the isotopic values has been added "(δ13C: -15 to -30‰, precision 0.1‰ δ15N: -1.1 to +32.7‰, precision 0.2‰."

Results:

In general, all results sections can be shortened. Quite often they contain repetitions, first describing general trends and then repeating them with numbers. I also don't like the style how the isotope data are described. Sometimes they are reported as "water O-isotopic signal", then a depletion of one isotope is mentioned in the same sentence with a simple delta notation. For reasons of clarity and to make it easier for the reader I suggest to use exclusively low or high delta values.

» We wanted to avoid endless repeats of δ18O, and were therefore using synonyms that are widely used and accepted. We have tried, however, to accommodate the comments of the reviewer by removing some of the synonyms and implement the suggestion.

P. 10552, l. 13-14: The distances displayed here do not match with the scale in the inset maps in figure 1. Correct this.

» In the updated manuscript, for both systems the distances in the plots are distance

along the river, within the floodplain. This has been updated in the figure axes, and in the caption of Fig. 1 ("Sampling stations will be further presented in distance along the river (km)."

P. 10552, l. 11-16: Why should river constrictions lead to a discharge minimum? One should expect higher velocities/discharge in such a case. The following sentences provide an explanation, but this phrasing is a bit unfortunate.

» River constrictions in a valley or steep channel would indeed lead to higher velocities, but on the floodplains the water can leave the normal river bed, and spread laterally onto the floodplain. In the Kafue Flats, we observed a reduction of flow velocity, caused by the resistance of the adjacent reeds (slack flow effect). The section has been rephrased: "During the wet season, the runoff in the main channel of both floodplains was characterized by a discharge minimum roughly in the middle of both systems (Fig. 3). Located around 100 km and 200-300 km downstream in the Barotse Plains and in the Kafue Flats, respectively, constrictions in the river bed were present, which promoted flooding of the surrounding floodplain area (Zurbrügg et al., 2012)."

P. 10552, l. 19-20: What do you mean by "upstream Zambezi", the Barotse plains? Then say so.

» Rephrased: "…Zambezi crossing the Barotse Plains…"

P. 10553, l. 16: What is meant by "organic nutrient species"? Carbon as such is not a nutrient.

»Changed to "organic carbon and nitrogen species".

P. 10553, l. 23: Interesting to read about loads. But the reader does not get any information how and for which points/locations these loads were calculated. This information must be included.

»The section on loads has been extended with the information on how the loads were calculated, and some of the descriptive sections that were previously in the discussion.

This has resulted in the following paragraphs, replacing P10553 L23-28: “Loads were calculated from the discharge and concentration data for the respective species, as the water column was well mixed (see Supplementary Information for details). Total carbon and nitrogen loads increased along the Barotse Plains during the wet season, mainly due to larger contribution by the dissolved organic form (Fig. 5). The increase in total carbon load in the Kafue Flats during the wet season was mainly attributed to the dissolved inorganic fraction. The magnitude of the wet season carbon loads leaving the floodplain area is comparable between the two systems (roughly 1500 t C d-1, Fig. 5), while the nitrogen loads in the Barotse Plains were almost twice as high as those in the Kafue Flats (44 t N d-1 and 20 t N d-1) During the dry season the loads decrease slightly. Net export was determined as the difference between the load at the downstream end of the floodplain and the load at the upstream end of the floodplain (Table 1). During the wet season, the Barotse Plains were a sink for all particulate phases, while the Kafue Flats acted as a source (Table 1). Both systems were sources of DOC and DIC. Dissolved organic nitrogen was exported from both floodplains, but the Barotse Plains retained the small DIN flux, while the Kafue Flats were a minor source. During the dry season, the Barotse Plains acted as source of particulate matter. For the Kafue Flats this could not be determined due to lack of POC and PN measurements in the downstream stretches of the river. DOC and DIC were retained by both systems. The Barotse Plains were a minor source of dissolved nitrogen, while the Kafue Flats retained both organic and inorganic nitrogen.”

P. 10554, l. 15-19: The C/N ratio of 166 looks rather strange and results from only two measurements. Although you may find a statistically significant difference, I don’t think that it has a real diagnostic value.

» While this value does indeed only stem from 2 measurements, the measurement procedures are reliable, and both measurements resulted in a C:N ratio an order of magnitude larger than during the wet season as a consequence of very low DON values. As there is no indication that these are outliers, we prefer to document these

values.

Discussion:

P. 10555: The whole discussion on the relevance of hydrology and inundation dynamics is absolutely not convincing. It is based on a mass balance approach with isotope data from another study for one of the flats and almost no data for the other flat. What is necessary for such a discussion is the volume of water moved and the period of inundation. From the discharge data and area of the floodplains the authors have, they should at least be able to calculate roughly the volume of water transported.

» A new initial paragraph has been added to section 5.1: “The discharge patterns (Fig. 3) showed how the bankfull capacity of the Zambezi and Kafue Rivers varied along the floodplain stretch. In both systems water has moved from the main channel onto the floodplain, roughly 600 and 400 m3 s-1 in the Barotse Plains and the Kafue Flats, respectively. Where the capacity of the channel increases again further downstream, water from the floodplains (and potential tributaries) returned to the main channel at higher rates. On the floodplain, flow velocities were extremely low (< 1 mm s-1 on the Kafue Flats in May 2008), which led to prolonged residence times of the water on the floodplain, during which evaporation might occur, resulting in heavier δ18O signatures in water that has spent time on the floodplain.”

» It has been stressed more thoroughly that the mass balance approach for the Barotse Plains is a first approximation (replacement of P1055,L5-7), and should be considered as such. “Logistical constraints prevented the collection of similar remote floodplain samples in the Barotse Plains. Assuming a similar floodplain signal in the Barotse Plains as in the Kafue Flats, a first approximation was made to determine how much water in the Barotse Plains has spent time on the floodplain.”

P. 10555, l. 20-21: What is meant here "with a floodplain contribution of 16%"? 16% of what coming from where? And following, what is meant by "there was still exchange between the river channel and some permanently inundated areas"? Could it be that lower discharge during the dry season is only related to evaporation? The paragraph then ends with a few examples from other areas/studies. But what is the inference with respect to own findings?

» This has been rephrased: "During the dry season, the increasing discharge along the Barotse Plains is most likely caused by inflow of the Luanginga tributary. By contrast, the decreasing discharge in the Kafue Flats combined with a calculated 16% of the downstream discharge having spent time on the floodplain (Zurbrügg et al., 2012) indicated that there was still exchange between the river channel and some permanently inundated areas in the downstream reaches of the Kafue Flats."

The whole chapter "5.2 Export and retention behavior" is quite disappointing, because it simply repeats overall results and then speculates about the reasons for the observed behavior. It should be reduced drastically or needs a real discussion of the factors mentioned.

» The results presented in this section have been moved to section 4.2 and the section has been rewritten to provide an argument about primary production and respiration occurring on the floodplain and in the river channel (5.2 Seasonality of C and N export and retention). The updated version of this section is as follows: "During the wet season, the Barotse Plains were characterized by a net export of dissolved phases and retention of particulate matter. Degradation processes or settling of particulate organic matter, either in the main channel or on the floodplain could result in apparent retention of POC and PN. The concurrent export of DOC, DIC, and DON could similarly be a result of degradation, or of leaching of vegetation or soils. During the dry season, the patterns were reversed, indicative of primary production on the Plains. In contrast, the Kafue Flats were a net source for both particulate and dissolved phases during the wet season, indicating a different balance. The high proportion of DIC to the net dissolved C export suggests that degradation was a dominant process during flooding. While the constant POC:PN ratios contradict large soil inputs, a combination of primary production in the main channel, and degradation and leaching of soil

and vegetation from the inundated floodplain (indicated by low oxygen concentrations of the water from the floodplain (Zurbrügg et al., 2012)) could be responsible for the observed patterns. During the dry season, the retention of DOC, DIC, DON, and DIN pointed towards primary production and potentially a minor contribution from sorption of dissolved organic phases onto particulate material. The observed net export of particulate organic matter might not have effects beyond the downstream reservoirs of Lake Kariba and Kafue Gorge, respectively (Fig. 1). Both impoundments will trap mobilized particles, and retain 70% and 90% of incoming total N and P within Lake Kariba (Kunz et al., 2011a). Nevertheless, on a catchment scale, mobilization of particulate organic matter from the inundated area of the river-floodplain systems resulted in specific POC and PON yields (net export per inundated area per year; Table 2) from the Barotse Plains, which were close to an order of magnitude higher than previously reported values for the entire Zambezi River (Beusen et al., 2005;Mayorga et al., 2010). Despite the behavior as a sink during the wet season, the normalization to inundated area has resulted in positive annual export from the floodplain. Also, DOC yields from the Barotse Plains were higher than previously estimated for the Zambezi, but comparable to those measured in the Amazon and Orinoco rivers (Table 2; Beusen et al. (2005); Harrison et al. (2005); Lewis and Saunders (1989)). Similarly, DON yields from the pristine floodplain were similar to values measured in the Amazon and Orinoco (Table 2). The Kafue Flats show negative DOC, DON, DIN yields, i.e. are retaining these species. These negative yields show how floodplains can impact the riverine loads in trends opposite to those observed for the whole catchment. Similarly, the high yields from the Barotse Plains underlined the dominant role of floodplains as biogeochemical reactors in riverine transport of organic matter from land to sea."

P. 10556, l. 14-17: What is the use of discussing concentrations in a chapter that deals with fluxes? This can be deleted.

» The marked difference between DIC concentrations during different seasons in the Barotse Plains was responsible for the smaller difference in net export between the seasons compared to the Kafue Flats. This has been removed.

P. 10556, l. 19: ": : :Kafue flats were a minor source."? If I can believe table 1 the Kafue flats should be a minor sink for DIN (I read table 1 as follows: wet season 0.1 t N d-1, dry season -0.2 t N d-1; taken together a negative flux should mean retention; right?). It would be helpful to make clear in the legend of table 1 what positive and negative signs mean, i.e. export vs. retention.

»We prefer discussing the seasonality, by separating the observations from the wet and dry season. This paragraph as a whole deals with the wet season, hence the statement that the Kafue Flats were a minor source (0.1 tN d-1). The dry season is discussed in the next paragraph, starting on P10556-L24. An explanation of the calculation of the net export has been added in section 4.2: “Net export was determined as the difference between the load at the downstream end of the floodplain and the load at the upstream end of the floodplain.” Additionally, the caption of Table 1 has been updated: “Net export (in t C d-1 and t N d-1), calculated as the difference between loads at the downstream and upstream ends of the respective floodplain, from the two floodplains during wet and dry seasons. Positive numbers indicate that the floodplain acted as a source (export), negative numbers indicate the floodplain acting as a sink (retention). POC and PN export from the Kafue Flats during the dry season could not be estimated due to lack of measurements at downstream locations.”

P. 10556, l. 19-23: This is hard to understand, the mixing of fluxes and loads and differences between numbers in table 1 and figure 5. I suppose the numbers are correct, but the way it is written up is confusing.

» We feel confident that the source of the confusion has been removed now that the results part (relating to Fig. 5, including L19-23) has been moved to section 4.2.

P. 10556, l. 24-25: This is an interesting point, but as is nothing but speculation. It belongs to the issues mentioned above. If the reader should believe in the relevance of Aeolian input, there must be some facts and discussion underpinning it, not just a

"most likely" without discussion.

& P. 10556, l. 27 ff.: Same issue: "DOC and DIC were retained by both systems, potentially converted: : :"; just speculation. Provide arguments or delete this. The same holds true for the following statement on p. 10557, l. 3-6.

» We’ve attempted to provide more arguments fur the observed patterns and feel confident that the updated version of the entire section (as mentioned above) has removed much of the speculation.

P. 10557, l. 7-10: This is an interesting point and relates to one of the major issues of the whole paper, the floodplain as a biogeochemical reactor. However, what comes is just a comparison to a few other systems and absolutely no discussion of the "biogeochemical reactor". So, what is happening there? What are the inferences for your own findings?

» The inclusion of a more process-based explanation for the observed patterns strengthens the discussion of the biogeochemical reactor. In the updated section (see above), the seasonal inferences are now stated more clearly.

P. 10557, l. 19-21: If I can believe table 1 the Barotse Plains are a net sink for particulate matter. How shall we understand the statement that the "particulate matter mobilized in the floodplain will end up in the sediments of Lake Kariba"? Again, looking at table 1 you may have some mobilization during the dry season, but what would be important to discuss why you find large seasonal differences. You have very interesting findings, but do not discuss them.

» The Barotse Plains are indeed a sink for particulate matter, but when considered as yearly yields per inundated area, the results (Table 2) show that it is actually a source, and we were referring to this material being trapped in Lake Kariba. I think some confusion has arisen from the use of terminology and the information presented in Tables 1 and 2. We’ve attempted to clarify this as need throughout the updated manuscript, and hope to have removed the source of the confusion.

P. 10557, l. 25-27: This is rather a commonplace. What I am missing are the inferences for the studied floodplains and what that could mean for the whole river catchment in terms of export vs. retention. What are the controls? How do they vary by season? Are the floodplains a sink or a source and is that different from other rivers?

» The commonplace sentence has been somewhat altered and relocated to make it more specific to the Zambezi, with the paragraph now ending with: "These negative yields show how floodplains can impact the riverine loads in trends opposite to those observed for the whole catchment. Similarly, the high yields from the Barotse Plains underlined the dominant role of floodplains as biogeochemical reactors in riverine transport of organic matter from land to sea." It has been our goal to investigate the behavior of the floodplain during different seasons, and how this relates to the whole catchment (Table 2). The seasonal variability and its potential controls have been more extensively described in the updated manuscript (see updated section above). Comparison of our floodplains to other rivers is not very feasible, because very few comparable studies of tropical river-floodplain systems have been published, and comparing our results to temperate or arctic floodplains would introduce too many variables.

The style of discussion in the following chapters is rather annoying, every paragraph starts with a conclusion and then tries to bring some arguments for it. In a scientific paper own findings should be presented in context with other studies/areas/findings and discussed and then a conclusion can be drawn on own findings. This is something the authors should correct.

» We feel it is important to stress the key message of a paragraph early on. This helps the reader navigate text. However, we have varied the style throughout the section.

P. 10558, chapter 5.3.1: This whole chapter does not discuss sources of dissolved organic matter as announced in the chapter heading "5.3 Sources of organic matter". It only once mentions runoff from inundated soils, the rest is seasonal variations without

further source discussion.

» We felt that this section did discuss the different sources of DOM during the contrasting seasons. However, it has been extended, following the adjustments in section 5.2. A paragraph has been added, that builds on the argument provided in section 5.2 "Based on the export and retention behavior of the two floodplains, degradation of floodplain-derived organic matter may be a large source of DOC in the Barotse Plains during the wet season. During the dry season, higher primary production and sorption of dissolved organic phases to particles may have decreased the DOC concentrations. In the Kafue Flats, degradation of organic matter on the floodplain was contributing to in-stream DOC during the wet season, whereas during the dry season, similarly to the Barotse Plains, primary production and sorption of dissolved phases onto particles were lowering DOC and DON concentrations." Additionally, the paragraph on the DOC:DON ratio has been moved from section 5.3.2 (P10559, L8-18) to section 5.3.1.

P. 10558, l. 11: Do not use the term "can explain" (here and throughout the whole manuscript!). A person can explain something, but the "source of DON" cannot explain something.

» This has been rephrased in the text.

Chapters 5.3.2 and 5.3.3 should be merged.

» The two sections have been merged to a section called "Particulate organic matter".

As is they are parameter discussions, but not source discussions. The C/N ratio alone is not suitable for such a source discussion. It is affected by numerous factors, the initial difference between plankton and plants/soils is just one of them. Selective decomposition of OM increases the C/N ratio over time. Adsorption of inorganic nitrogen to fine-grained particulate matter (clays) leads to a low C/N ratio.

» Visually, there is very little clay in the suspended particulate matter in both systems; especially in the Barotse Plains, the surroundings are mainly sandy (geological formation: Kalahari Sands).

The C/N range of terrestrial plants and soils is huge. Similarly, using the stable carbon isotope composition of OM alone to distinguish between sources is also of limited value. Also, just using it in terms of contributions of C3 and C4 plant material means ignoring all the other sources/factors that contribute to the d13Corg of a specific sample one considers. What you mainly find in river suspensions is not plant debris, but eroded soil (including plant debris and processed plant debris). The d13Corg of soil can be very different from the plant growing on it. Moreover, in the water you find micro- and macrophytes the d13Corg of which varies over a wide range. This can be very important in a river and in particular in floodplains. Of course, you can have a lot of microalgae (or not) depending on nutrients and turbidity etc. Moreover, I have myself observed macroalgae and water hyacinths in massive amounts in tropical rivers and reservoirs, all of them aquatic plants with a d13Corg that falls in the range between terrestrial C3 and C4 plants. All this is not mentioned in the discussion in this manuscript. And what is also mandatory for such a discussion is to have stable isotope data from the various OM sources you find in the area, not just literature data. Are there no stable isotope data available for the soils and terrestrial and aquatic plants from the area?

» We have made no observations of macroalgae or water hyacinths. Initial results from a large effort of vegetation sampling in June 2015 in the Barotse Plains showed very little ∂13C values intermediate between C3 and C4, further indicating that these water plants were of limited importance in our system. We do however have the following information about the various sources of organic matter for the Barotse Plains and the Kafue Flats (previously published in the supplementary material of Zurbrügg et al. (2013)): » Soils: Barotse Plains -12.3 and -22.9‰ Kafue Flats -20.3$\pm$2.5 ‰ » Trees: on the Barotse Plains -28.3 $\pm$ 1.22 ‰ (average of 6 different species) » Most abundant reeds: Barotse Plains -25.9, -12.8, -27.3 ‰ Kafue Flats -13.3 and -25.5‰ From these numbers we perceive that any floodplain input, whether this was soil- or vegetation-derived, would result in heavier ∂13C values. We have restructured the two sections

to focus on seasonal floodplain inputs in contrast to material from more permanent terrestrial vegetation.

Of course, if these data are not available, you have to use literature data, but these are often of limited value. Some interesting information from the area (resulting from other publications of the same group) is given (p. 10560, l. 11-13 and l. 19-26), but the own data are hardly discussed in the context of those observations. Take this up and develop the scenario which explains the observations made.

» In the updated manuscript, the data mentioned above has been introduced, along with a more generalized argument on floodplain inputs. The new section includes both C:N and ∂13C data, and more of a scenario to explain the observations: "The higher C:N ratio of the suspended matter in the Barotse Plains year-round indicates a soil-derived source in the pristine part of the catchment. In contrast, C:N ratios found in the Kafue Flats during the wet season were indicative of aquatic production (Zurbrügg et al., 2013). This could be attributed to the presence of the ITT reservoir: surface sediments from the reservoir showed an elevated C:N ratio (12.1$\pm$0.6, Supplementary information of Zurbrügg et al. (2013)), similar to the numbers found for the suspended matter in the Barotse Plains. Hence, the presence of the dam significantly affected the chemical composition of the suspended matter, and while soil-derived suspended matter settled in the reservoir, mainly photosynthetically produced organic matter from the reservoir surface waters reached the Kafue Flats and eventually the Kafue-Zambezi confluence. The decrease in C:N ratio along the floodplain in the Kafue Flats during the dry season could be indicative of gradual organic matter input from nitrogen-fixating vegetation. As a consequence of nutrient elimination in the ITT reservoir, widespread encroachment of N-fixing woody plants onto the floodplain has been observed (Blaser, 2013). While the C:N ratio showed little variation throughout the year in the Barotse Plains, the stable C-isotopic signatures of the particulate matter further suggest different contributors to the POC in the river. During the wet season, the particulate organic matter in the Barotse Plains is 13C enriched compared to the dry season (-26.9 and

-28.5‰ respectively). Organic matter sources on the floodplain (soils on average -18‰ abundant reeds between -12 and -27‰ unpublished data) had distinctly heavier δ13C signatures than the permanent vegetation in the area (average of 6 different tree species -28.3 $\pm$ 1.22 ‰ unpublished data). Inputs from permanent vegetation were the dominant source of organic matter during the dry season, whereas inputs from the floodplain during the wet season led to more enriched values. Shifts to isotopically heavier organic matter during the wet season as observed in the Barotse Plains, have been described for the Tana River in Kenya (Tamooh et al., 2014), the Sanaga River in Cameroon (Bird et al., 1998), and the Congo River in Central Africa (Mariotti et al., 1991). These studies clearly showed how the source of organic matter transported by tropical rivers is changing with inundation. In contrast, the particulate organic matter in the Kafue Flats was more enriched during the dry season compared to the wet season (-26.5 and -28.5‰ respectively). The average dry season δ13C value for the Kafue Flats should be interpreted with caution, since there is a clear spatial pattern: values become more depleted towards the end of the floodplain. This spatial pattern has previously been attributed to floodplain-derived particulate organic matter, which would consist of phytoplankton and periphyton material in the permanently inundated area in the downstream reaches of this floodplain (Zurbrügg et al., 2013). In the more typical stretch of the floodplain however, the dry season value was even heavier. The encroaching species have resulted in a vegetation pattern with C4 species occurring close to the river, and C3 species growing on the higher grounds that are only seasonally flooded (Blaser, 2013;Ellenbroek, 1987). The inputs from these encroaching species can be considered as terrestrial inputs of permanent vegetation. The difference in composition and origin between dissolved and particulate phases, i.e. DOM from terrestrial sources, POM more aquatic influence has previously been described for the Amazon (Aufdenkampe et al., 2007;Hedges et al., 1986) and the Fly-Strickland system in Papua New Guinea (Alin et al., 2008). We showed that the interaction of the river with its floodplain is responsible for the changes observed in organic matter characteristics, but that influence of aquatic production in the systems only originated

from the reservoir. "

Chapter "6 Conclusions" is quite long and rather a summary with only a few conclusions. If it should stay like that, it should be called "Summary and conclusions". Moreover, in some places it is not clear and/or simply wrong.

»The conclusion section has been shortened and more focused towards the conclusions of the study.

P. 10561, l. 14-15: What is meant by "yields"? Do you want to say that the Barotse Plains have a higher relative export per unit area and time? Then say so.

» The sentences relating to yields have been modified to express the net export rates from the floodplain, normalized to floodplain area.

P. 10561, l. 18-19: What is this? If I take a look at table 1 the Kafue Flats are doing exactly the opposite, with a small retention during the dry season and a high export during the wet season they seem to be a net sink for dissolved and particulate matter on an annual basis.

» The reviewer refers to seasonal information from Table 1, while we were trying to convey the annual average as documented in Table 2. As mentioned previously in the discussion about section 5.2, we have taken steps to eliminate the confusion between information from Table 1 and 2.

P. 10561, l. 19-23: see my previous comment. This is one possibility or part of the explanation, but not the whole truth.

» This part has been updated in accordance with the changes made in the discussion: "Particulate organic carbon δ13C values indicated a larger contribution of floodplain-derived organic matter in the Barotse Plains than in the Kafue Flats during the wet season, and the reversed situation during the dry season. The spatial distribution of C3 and C4 plants in the floodplains disrupts the signal of floodplain inputs during the wet season in the Kafue Flats."

P. 10561, l. 24 ff.: As mentioned before, this is just part of the story and therefore cannot remain as a conclusion.

» While it is part of the story, we feel it is important to stress how changes in hydrology can have a secondary effect on longer time scales, that impacts the river-floodplain biogeochemistry. In the conclusions this has been included as the last sentence “This is a result of the presence of the dams that only became evident with time, and shows the importance for monitoring after dam construction.”

P. 10562, l. 3-7: This is definitely wrong. The seasonal difference in C/N ratios is fairly small and it is absolutely in the range of aquatic plants. Of course, variations in soil OM contributions may play a role, but there may be other factors (see above).

» The sentence may indeed be misleading. Sediments in the ITT reservoir have C:N ratios of 12, very much in the range of soils reported in the Kafue catchment (around 15; Zurbrügg et al. (2013)), whereas the water exiting the reservoir has a distinctly (and statistically significant) different signature of 8-10. A few sentences have been added after P10559, L7: “The decrease in C:N ratio along the floodplain in the Kafue Flats during the dry season could be indicative of organic matter input from nitrogen-fixating vegetation. As a consequence of nutrient elimination in the ITT reservoir, encroachment of N-fixing woody plants onto the floodplain has been observed (Blaser, 2013).”

P. 10562, l. 9-14: This is an interesting point, however, the reservoir effects were hardly discussed before and this seems to be rather a conclusion of the cited paper.

» The reservoir effects have been extended in the discussion section, and the lines in the conclusion section have been merged into “Differences between the two systems that can be attributed to the presence of the Itezhi-Tezhi reservoir upstream of the Kafue Flats included a delay of the input of runoff-derived floodplain soil organic matter and. . ..”

P. 10562, l. 15-16: No, it has not been shown. It has just been mentioned and was not

really discussed. As such it is just an unjustified assertion. Nevertheless, I agree with the authors that this could be an important factor.

» We are convinced that the improved line of evidence discussed in sections 5.2 and 5.3 now justifies conclusions along this line, and in the updated manuscript this sentence has been removed.

P. 10562, l. 17-19: No. The only difference one can see is in the seasonal discharge pattern and the change in the Kafue Flats before and after dam construction. However, the data presented in this study do not give any hint on specific transport or export pulses of dissolved and particulate matter besides the general dry vs. wet season differences.

» We have shown that the timing of DOC and DON inputs to Kafue Flats deviate from an established process, which is visible in the Barotse Plains. Combining that observation with previously published seasonal patterns has led to the conclusion that this must be an artifact of the presence of the dam. It has however been rephrased in the updated manuscript.

Tables:

Table 1: Clearly designating positive and negative signs to the terms "export" and "retention" (or similar) would help the reader to understand easily what is meant when the text refers to the numbers in the table.

» The table caption has been updated to accommodate this.

Table 2: What is the use of this table? The numbers are to some extent mentioned in the text, but not really discussed. Why were (only) these rivers chosen and not others, for example, non-tropical rivers? Of course, it contains interesting information, but it is hardly used in the discussion. If it will remain as marginal as is now, it is sufficient to mention the numbers in the text and delete the table.

» For comparison, we have chosen only large tropical rivers for which the relevant data was available, in order to place the values obtained for the floodplains into a broader perspective of similar systems. We feel that comparisons to other, non-tropical rivers and/or floodplains would require a comprehensive global review, which was well beyond the scope of our study. The comparisons between the various systems are presented in the text, and then the interested reader can refer to the table for the exact numbers.

Figures:

In general, I find the choice of colors and symbols a bit unfortunate, the differences are fairly small (e.g., black vs. dark blue) and they do not allow to distinguish easily.

» The colors in the various figures have been modified, see below for details. In addition, Fig. 3 has been modified to dark blue and orange as well.

Figure 1: The distance scales in the floodplain insets do not match with distances mentioned in the text. What do the red symbols represent?

» The red symbols represent the large dams in the catchment; a statement to this end has been added to the figure caption ("Map of the Zambezi catchment, with floodplains (in green) and large dams (red arrows) marked.").

Figure 4: I think, what you have there is not PON, but PN.

» This has been corrected. The color scheme has been updated: particulate = dark blue, dissolved organic = orange, dissolved inorganic = light green.

Figure 5: Very hard to figure out what is what. Do not use grey scales, but color. In the case of nitrogen (figures e-h), does it mean there is no dissolved inorganic nitrogen?

» The colors of this graph have been modified in the same fashion as figure 4. There was indeed very little DIN, with measurement values close to or below detection limits.

Figure 6: Symbols and color are too similar to distinguish easily. Be careful with the legend. What you show is the ratio of particulate organic carbon to particulate total

nitrogen, but the ratio of dissolved organic carbon to dissolved organic nitrogen, right?

» In order to better distinguish the two series, the square symbols have been replaced by triangles, and the colors have been altered (to dark blue and orange). The axes titles have been modified to POC:PN, DOC:DON, and ∂13CPOC (‰).

Figure 7: I like the idea of having some kind of visual summary! However, as a reader I would prefer not to see the numbers again, but instead I would like to see the differences in the various sources/processes, e.g. export/retention, terrestrial/aquatic OM etc.

» We consider that the expectations for a visual summary are a very personal matter, looking at the contrasting sentiments expressed by the two reviewers. To avoid repetition of the numbers, these have now been lumped together into POM and DOM, and the removal or export processes are represented with arrows proportional to the size of the flux.

References:

Zurbrügg, R., Suter, S., Lehmann, M.F., Wehrli, B., and Senn, D.B. (2013). Organic carbon and nitrogen export from a tropical dam-impacted floodplain system. Biogeosciences 10, 23-38. doi: 10.5194/bg-10-23-2013.

Zurbrügg, R., Wamulume, J., Kamanga, R., Wehrli, B., and Senn, D.B. (2012). River-floodplain exchange and its effects on the fluvial oxygen regime in a large tropical river system (Kafue Flats, Zambia). Journal of Geophysical Research 117, G03008. doi: 10.1029/2011jg001853.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C6090/2015/bgd-12-C6090-2015-supplement.pdf